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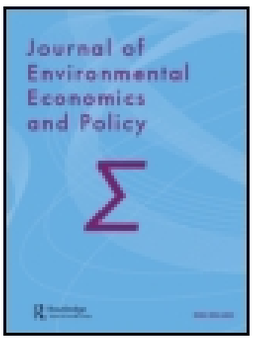

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Valuing malaria morbidity: results from a global meta-analysis

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ABSTRACT

The risk of malaria transmission worldwide is expected to increase with climate change. In order to estimate the welfare implications, we analyse the factors that explain willingness to pay to avoid malaria morbidity using a meta-analysis. We fail to replicate a previous meta-analysis, despite using a near-identical dataset. Thus, this paper outlines a more robust approach to analysing such data. We compare multiple regression models via a cross-validation exercise to assess best fit, the first in the meta-analysis literature to do so. Weighted random effects gives best fit. Confirming previous studies, we find that revealed preferences are significantly lower than stated preferences; and that there is no significant difference in the willingness to pay for policies that prevent (pre-morbidity) or treat malaria (post-morbidity). We add two new results to the morbidity literature: (1) Age has a non-linear impact on mean willingness to pay and (2) willingness to pay decreases if malaria policies target communities instead of individual households.

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Malaria; meta-analysis; morbidity; stated preferences; willingness-to-pay

1. Introduction

Since 2000, the world has seen a general decline in the rate of malaria transmission. Through benchmarks, such as the Millennium Development Goals, and programmes, such as Roll Back Malaria (RBM), malaria mortality rates dropped by 42 per cent between 2000 and 2012. This is in line with meeting the WHO targets for malaria, which is a 75 per cent reduction by 2015 (Tuschman 2013). However, recent developments are threatening to undo this progress. For example, it has been well-documented that malaria is sensitive to weather variations and climate change (Bouma and Kaay 1996). This implies that the risk of malaria transmission may increase with climate change in certain regions (Patz et al. 2002; McMichael, Woodruff, and Hales 2006), as also reported by the Intergovernmental Panel on Climate Change (IPCC 2014).

In order to design effective policies against malaria, cost-benefit analysis (CBA) helps in evaluating alternative courses of action (Mills, Lubell, and Hanson 2008). This paper focuses on the benefits of reduced malaria incident rates. Willingness to pay is *a* measure of the monetary value of the utility differential caused by an alternative health state (Brouwer and Bateman 2005). We focus on the willingness to pay for reduced malaria morbidity.

The valuation literature has seen a surge in studies that measure WTP to avoid or treat various diseases, including malaria. For malaria, WTP studies can be found from 1993 (Weaver et al. 1993)

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until the present (Aleme, Girma, and Fentahun 2014). An effective method of summarizing these studies is to systematically analyse their results in a meta-analysis. Meta-analysis is a statistical approach to synthesize the main findings from different studies focusing on a similar phenomenon or target variable, and identifying sources of variation in their measurement (Van Houtven 2008). In this case, we focus specifically on the measurement of WTP to avoid or treat malaria. A meta-analysis has the distinct advantage in that it avoids potential researcher selection bias when one summarizes measurements across the literature. Additionally, meta-analysis facilitates the transfer of benefit values across different settings (Brouwer 2000).

The literature concerning WTP to avoid malaria morbidity has already been summarized by Trapero-Bertran et al. (2012). Our study builds upon that existing study and adds to it in a number of ways: We extend the database with other studies, add new explanatory factors and improve the econometric framework. Our main objective is to explain the differences in average WTP to avoid morbidity risk due to malaria, using a meta-analysis.

The average WTP value from the individual malaria valuation studies is the dependent variable. Using regression analysis, we examine to what extent methodology-related (e.g. revealed preferences versus stated preferences), sample population-related (e.g. age, income) and policy-related (e.g. specific treatment) explanatory variables have a systematic impact on WTP to avoid malaria morbidity.

The rest of the paper is organized as follows: Section 2 provides an overview of the methodology and previous meta-analyses, Section 3 explains the data collection procedure, Section 4 summarizes the data, Section 5 presents the analysis and Section 6 concludes.

2. Meta-analysis

2.1. Methodology

A meta-analysis aims to systematically describe empirical findings. A number of publications give guidelines to constructing datasets and analysing them (Stanley 2001; Smith and Pattanayak 2002). We follow Van Houtven (2008) and Nelson and Kennedy (2009). Nelson and Kennedy (2009) reviews 140 published valuation meta-analyses in terms of 5 aspects: (i) sample selection criteria, (ii) basic data summary, (iii) primary data heterogeneity, (iv) treatment of heteroskedasticity and (v) robustness checks. Van Houtven (2008) describes how these aspects should be applied to datasets when the variable of interest is WTP for health outcomes.

The sample selection criterion is concerned with author or publication bias occurring during the creation of the dataset. If the researcher is getting studies through citations in a few papers, then the dataset may be biased in favour of published or publishable results. When collecting data, there must be a standard search process that prevents such biases from occurring. This involves explicitly specifying the keywords and databases searched, along with how articles are selected. Out of the 140 meta-analyses, only 61 mention a selection criteria (Nelson and Kennedy 2009). In health valuation this is of particular importance, since there is relatively little uniformity in research techniques across studies. A broad selection criterion may give too many different studies to compare. On the other hand, a restrictive selection criterion may give too few studies for a meaningful analysis (Van Houtven 2008).

The basic data summary category is concerned with the explanation of the dataset itself. As in any empirical study, descriptive statistics and scatter plots of key variables help strengthen its validity. In a meta-analysis, since each observation carries a standard error with it, these standard errors can be weighted when making descriptive calculations. Standard errors are also used as weights in regressions (Van Houtven 2008). Additionally, these weights allow for more accurate descriptives to be presented. This is not a common practice in economics papers, but it is in medicine meta-analyses. Hence only 14 of the 140 meta-analyses report weighted statistics (Nelson and Kennedy 2009).

Primary data heterogeneity occurs because the observations come from different studies. This implies that each observation carries some (un)observed characteristic of the particular study from which it was drawn. Regression models aim to control for this heterogeneity. Being able to

account for this heterogeneity, one can explicitly show how (and perhaps why) empirical results of the same nature differ from study to study (Van Houtven 2008). Thirty-three of the 140 meta-analyses make use of OLS models that do not account for between-study heterogeneity (Nelson and Kennedy 2009).

Since observations are subject to heterogeneity, this means that the resulting variance of residuals is not constant across observations. In other words, the regression model may suffer from heteroskedasticity. If one controls for heterogeneity, then this should not be a problem (Van Houtven 2008). It is also possible to approach this problem via robust or clustered standard errors. Robust standard error algorithms, such as the Huber–White estimator (White 1980), are sufficient to deal with heteroskedasticity. Interestingly however, 46 of the 140 studies do not treat heteroskedasticity in their regression framework (Nelson and Kennedy 2009).

The final aspect of evaluation is robustness checks. Robustness checks in applied econometrics are considered a 'tenth commandment' (Kennedy 2002). This can be done by implementing different functional specifications in regressions (Van Houtven 2008), excluding outliers and alternating regression models (Nelson and Kennedy 2009). However, in the 140 papers that are reviewed by Nelson and Kennedy (2009), only 41 mention outliers; and of these 41, only 16 report a residual analysis. There is no mention of different functional forms (Nelson and Kennedy 2009).

With these issues in mind, we consider three regression models to be utilized in our data analysis. The 'first port of call' in meta-regression models is the WLS (Nelson and Kennedy 2009). The WLS is a case of the OLS where the residual variance is assumed to differ across observations. In principle, this difference is due to an observed statistic. All other assumptions of the OLS, such as the independence of the error on explanatory variables, do not change. Hence, the function (in a simple case) can be:

$$y_k = \beta_0 + \beta_1 x_{1k} + \beta_2 x_{2k} + \beta_3 x_{3k} + \epsilon_k, \tag{1}$$

where k is an individual observation and $\epsilon_k \sim N(0, \sigma_\epsilon^2/w_k)$, w_k being the cause of heteroskedasticity across observations. In our case, this is a study-specific variable, such as sample size. If this is true, then Equation (1) is efficient and gives unbiased coefficients. Determining w_k, especially for WTP observations, is not trivial. Standard practice is to use the standard error of each mean WTP observation as w_k. However, nothing is claimed about the heterogeneity across studies in this case. Here too, we make the claim that all heterogeneity information is stored in the standard errors, although this may be too restrictive (Van Houtven 2008).

If we want to analyse study effects, then we add in study dummies in Equation (1). A more systematic way of doing this is to assume that all studies are panels and then employ a panel model. With this, Equation (1) is expanded upon, and transforms to our second regression model:

$$y_{jk} = \beta_0 + \beta_1 x_{1jk} + \beta_2 x_{2jk} + \beta_3 x_{3jk} + \mu_j + \epsilon_{jk}. \tag{2}$$

The study index is denoted by j. Hence, observation k is found in study j. Here we assume $\epsilon_{jk} \sim N(0, \sigma_\epsilon^2)$, which is independent across explanatory variables and μ_j. Since we know that study heterogeneity exists, the panel effects term (μ_j) is considered to be random. Thus, $\mu_j \sim N(0, \sigma_\mu^2)$ and σ_μ^2 is the random effects coefficient. In principle, Equation (2) is sufficient to explain study heterogeneity. The Hausman test (Hausman 1978) can be employed to see if this effect is significant and, hence, random or fixed.

Note that the information on standard errors of mean WTP is now assumed to be in the random effects coefficient. The information of the standard errors can be incorporated by a weighting scheme. In Equation 2, we now assume $\epsilon_{jk} \sim N(0, \sigma_j^2)$. This new variance term, which changes across studies, is dependent on the standard error of mean WTP. In other words, we account for study-level differences and weighting of mean WTP. This is the model used in Trapero-Bertran et al. (2012).

This specific random effects model is sufficient to solve problems associated with meta-regressions. We control for study heterogeneity and the resulting heteroskedasticity. However, we are bounded by only having one level of grouping. In the random effects model, the μ_i variable is an addition to the constant term β_0, not the slope coefficients. What if observations are grouped into studies but studies are grouped in authors or countries? This requires accounting for grouping at a higher level. Additionally, what if we want to explicitly observe differences between groups using group-level variables? This means assuming that slope coefficients are random as well.

The mixed effects specification addresses both issues simultaneously. We expand Equation (2) to include an additional grouping level and random effects across explanatory variables. This expansion results in the third regression model of interest:

$$y_{ijk} = \beta_0 + \beta_1 x_{1ijk} + \beta_2 x_{2ij} + \beta_3 x_{3i} + \mu_i(x_{3i}) + \mu_i + \mu_{ij}(x_{2ij}) + \mu_{ij} + \epsilon_{ijk}. \quad (3)$$

Authors or countries are denoted by the index i. Hence, observation k is found in study j which was conducted in country or written by author i. Naturally, each study must have occurred in one country. Note that the x_1 series changes at the observation level. This is contrary to x_2 and x_3 which change at the study and author/country levels. μ_i and μ_{ij} are random effects across the groups, which affect the constant term β_0. Similarly, $\mu_{ij}(x_{2ij})$ and $\mu_i(x_{3i})$ are random effects across the variable series x_2 and x_3 respectively. They describe how the β_2 and β_3 slope coefficients change across groups. The error term has the standard assumption of $\epsilon_{ijk} \sim N(0, \sigma_\epsilon^2)$. All random effects are assumed to be independent of the error term and each other (Konstantopoulos 2006).

Equation (3) provides a flexible framework where the sources of heterogeneity and heteroskedasticity can be examined. If we know that study and author/country level variables are causing non-constant variance in the residuals, then we can account for these by adding in random effects. At the same time, we are able to capture any additional heterogeneity that may occur at levels other than the study. The strength of the model comes from its explanatory potential. While Equation (2) (and to some degree Equation (1) provides reliable output, they lack in explanation. Thus, we use the mixed effects model as our main regression specification.

In principle, the coefficients in Equation (3) are clean of any study and author/country effects. Therefore, we are able to utilize them in predicting the mean WTP to avoid malaria morbidity in other settings. This idea is the driving force behind benefit transfers: the benefit of avoiding malaria in a new policy setting can be estimated with the above-mentioned regression analysis (Kaul et al. 2013).

2.2. Existing disease valuation meta-analyses

Meta-analyses on the morbidity valuation of specific diseases are fairly recent. Before, there were many studies on WTP to avoid/reduce morbidity itself. For example, Johnson, Fries, and Banzhaf (1997) present a meta-analysis on how morbidity duration and severity impact valuation. WTP to avoid short-term morbidity is chosen as the variable of interest and is regressed on a health state index. After adjusting for between-study heterogeneity, the authors find that WTP is positively affected by severity and length of morbidity. Another example is Van Houtven et al. (2006), which conclude that WTP increases with duration but less than proportionally so.

Trapero-Bertran et al. (2012) present a meta-analysis on WTP for malaria treatment. All mean WTP figures are converted to 2011 US Dollars. Although they have 59 studies, only 24 of them report standard deviations, which are used for calculating weighted WTP values in the analysis. The regression model is a random effects model where the observations are weighted by their standard error and the panel is identified by study. Mean WTP is regressed on variables regarding the study design, location, policy and country-average GNI per capita. The regression results are checked for robustness by running the same specification on the same variables minus sample properties (rural/urban, years of education and country).

They conclude that mean WTP to avoid malaria increases with GNI per capita and is sensitive to the elicitation method used. Trapero-Bertran et al. (2012) underline the lack of data in malaria WTP

studies. One particular concern is the inclusion or exclusion of true zero WTP values in the average reported WTP. They implicitly link the heterogeneity across studies to the fact that crucial information regarding the calculation of estimated WTP is often left out in study descriptions.

2.3. Contributions of this study to the existing literature

Our contribution is fourfold: database extension, definition of explanatory variables, modelling framework and results. None of these are in the previous meta-analysis.

We cleaned the database of double-counts. We identified multiple studies that make use of the same dataset and report the same results. Some results were reported in a working paper and then a journal article. Sometimes, the same results were used to underline a different phenomenon. We accounted for this by cross-checking survey location and data years between studies with the same authors. If the location and year matched, then the studies were screened to determine whether or not the same dataset had been used.

We also added new explanatory variables. We include household income, payment frequencies, respondents' age and altruistic policies. In addition to this, we also include revealed preference studies.

We used PPP-adjusted values rather than exchange rate adjusted values. This is because most studies in the database were conducted in rural areas of developing countries. Hence, the households' purchasing power is most likely not reflected in the official currency market. Using exchange rate conversion would underestimate the actual WTP in dollar terms; hence we use PPP conversion for a better comparison. The same conversion procedure is used for household income.

Malaria prevention and treatment are used interchangeably in the previous meta-analysis. We do not, and check if there is a difference.

Meta-regressions are prone to be sensitive to model specification (Nelson and Kennedy 2009). We employ a mixed effects model and compare it to other models that are its special cases (WLS and weighted random effects). We test which model variant fits best, and run further specification tests.

We add new results to the determinants of WTP to prevent/treat malaria, taking our hypotheses based on the morbidity valuation and other economic valuation literature.

3. Data collection procedure

In collecting the data, an extensive literature search was conducted using Google Scholar, EconLit, IngentaConnect, JSTOR, PubMed and Web of Science databases. Five sets of keywords were utilized: 'Willingness to Pay Malaria', 'Contingent Valuation Malaria', 'Revealed Preferences Malaria', 'Economic Valuation Malaria' and 'Stated Preferences Malaria'. More often than not, a typical search returned more than 100 studies at a time. In Google Scholar, only the first 10 pages of results were considered (10 results per page). In other databases, the maximum number of articles considered per search result was 200.

80 papers were selected and 78 of them downloaded. The remaining two papers were identified by the search but were not accessible[1]. Emails were sent to the authors requesting the papers, but no reply was received, also not after sending reminders.

Twelve papers were eliminated because WTP was not estimated, and five did not report it. In five cases, the same study had been downloaded multiple times (working paper, technical report, journal publication, etc.). The final publication version was used. In another case, the same WTP for the same treatment policy from the same sample was used in different papers by the same authors. These too were discarded to avoid double-counting. However, WTP for different policies were treated as separate observations even when based on the same survey.

A total of 55 papers remained (see Table A3 in Appendix). Some of the eliminated studies were included in the analysis presented in Trapero-Bertran et al. (2012), so we cannot replicate their results.

Out of these 55 studies, 192 mean WTP values were extracted. Four of these values (Lertmaharit, Kamol-Ratanakul, and Saul 2000) could not be used, because the date of the data collection is not specified.

4. Data summary

4.1. Mean WTP to avoid malaria morbidity

Since avoiding malaria can be avoided in many ways (bed-nets, health-care, pesticides, etc.) with different payment frequencies, standardization is crucial. Every WTP and monetary figure is expressed in 2012 international US dollars (calculated with the Geary-Khamis method). The PPP conversion factor and GDP deflator data are taken from the World Bank. WTP is converted into a value per product or service offered. That is, if a paper reports a mean WTP of 100 dollars for 2 pesticide programmes, the number is divided by 2. In some WTP studies, respondents were asked for one-off payments, but we interpret these as repeat payments if the impact is transient. For example, we assume that payments for a malaria vaccine which is effective for 2 years only is renewed every two years. Payments for an indefinite vaccine are treated as a one-off payment. We use dummies for monthly, quarterly, yearly and one-off payments. We assume that payments are one-off, unless stated otherwise. Trapero-Bertran et al. (2012) did not standardize stated WTP values and so our results are hard to compare to theirs.

4.2. Key explanatory variables

The key explanatory variables were selected according to what was found in the morbidity, mortality and environmental valuation literature:

- Income: more income implies a higher WTP value (Asafu-Adjaye and Dzator 2003; Onwujekwe et al. 2006; Udezi, Usifoh, and Ihimekpen 2010). Additionally, we expect an inelastic relationship between income and mean WTP (Bosello, Roson, and Tol 2006).
- Revealed Preferences: WTP is reported to differ between revealed preferences and stated preferences (Kennedy 2002). People may overstate their WTP (Bateman et al. 1995). Hence, we expect a lower WTP if revealed preferences are used. Pooling revealed preference and stated preference data into the same regression is equivalent to merging Marshallian and Hicksian value estimates. This problem is overcome by coding revealed preference studies into the regression models through a dummy variable.
- Elicitation Method: Different methods often produce different values of WTP, with discrete choice methods producing higher values than open-ended questions or payment cards (Bateman et al. 1995). For an explanation of all CV elicitation methods used, see Table A1 in Appendix.
- Payment Frequency: One-off payments result in higher mean WTP values than annual payments (Loomis and White 1996) and monthly payments yield a higher WTP than annual payments (Spaninks and Hoevenagel 1995; Pearce et al. 2002).
- Inclusion of Zeros: Studies sometimes do not include true zero WTP values into the calculation of mean WTP. Trapero-Bertran et al. (2012) took this into consideration, but did not find a significant effect.
- Nigeria: More than half of our observations are from Nigeria, which was also the case for Trapero-Bertran et al. (2012). We therefore include a Nigeria dummy in the regression analysis. Note that studies done in Nigeria did not focus on a particular type of malaria prevention/treatment policy (all chi-squared tests yield p-values > 0.1).
- Treatment: We compare prevention and treatment. There are two approaches. The first stems from the health-state expected utility theory. Here, utility functions satisfy the von Neumann-Morgenstern axioms and their parameters vary between health states (Arrow 1974; Viscusi and

Evans 1990). The WTP value deriving from this framework does not account for the method of going from one health state to another. For example, a treatment that has a 90% likelihood of success and a prevention that has 90% effectiveness have identical WTPs, since the final expected outcomes are the same ('healthy' utility with 90% probability).

The second approach considers individuals being biased in their projection of different utility states. A healthy agent is more likely to over-project the loss of utility due to being sick, whereas a sick agent possibly under-projects the gains from being healthy. The healthy agent can be over-projecting due to loss aversion and the sick agent can be under-projecting because they have adapted to being sick (Loewenstein, O'Donoghue, and Rabin 2003; Dolan and Kahneman 2008). Under this approach, the above-mentioned prevention should have a higher WTP than the treatment policy.

- Control: The WTP for reductions of mortality risk changes with the perception of control. For example, WTP for reducing car accident death probability is less than WTP for reducing the risk of dying from bronchitis (Viscusi, Kip, and Huber 1987; Viscusi, Magat, and Huber 1991). We test whether the locus of control affects WTP, distinguishing between private and community interventions.
- Age: Age has a non-linear impact on mortality valuation, for reasons that are still under discussion (Krupnick 2007).
- Altruism: We define altruistic policies as those that have a benefit to the surrounding households. Thus, we expect free-riding to occur and hypothesize altruistic policies to be valued less than other policies.
- Publication Type: We test for differences between mean WTP values across different types of publications. Publication selection bias, i.e. only high WTP estimates being selected for journal publication, is a concern in meta-analyses. We compare WTP outcomes between journal publications and non-journal publications. If there is a bias, then WTP estimates from non-journal publications should be systematically lower than estimates from journal publications.

Table 1 lists our variables of interest and compares them to what has been tested in Trapero-Bertran et al. (2012). There is some common ground, but we add and take away from their variable list. For example, we do not include rural/urban and years of education. This is because we have included the household income variable, which can be influenced by years of education and location of the household. Moreover, education data is too noisy. Some papers give years of education, others indicators or dummies (e.g. literacy).

Table 1. Explanatory variables.

This paper	Trapero-Bertran et al. (2012)
Policy	
Altruism	
Control (Goods & Services)	
Payment frequencies	
Treatment	Treatment
	ITNs
	Other prevention
Sample	
Nigeria	Nigeria
Inclusion of zeros	Inclusion of zeros
Household income	GNI per Capita
Respondent age	
	Rural
	Rural or urban
	Years of education
Methodology	
CV elicitation method	CV elicitation method
Publication type	
Revealed preferences	

One important variable of interest is the effectiveness of each policy. This is not included because not all papers reported the effectiveness rate of their proposed interventions. We attempted to gather effectiveness information from external sources. However, the gathered data was too few, making a regression analysis non-viable. Therefore, we leave out the effectiveness of each policy of our database.

4.3. Descriptive statistics

Table 2 gives an overview of differences in mean WTP across subgroups. From left to right first the statistics weighted by standard errors, reported in the papers, are presented, followed by the non-weighted statistics. Although the weighted statistics are lower, the confidence intervals shows that this difference is not significant at the 5 per cent level. The weighting produces more consistent statistics, since information regarding study effects is incorporated. Table A2, in the appendix, presents descriptive statistics for all variables.

The overall mean WTP is close to 40 US Dollars per year (2012, PPP). This number is not statistically different from the mean WTP for only one-off policies. There is some preliminary support for our hypotheses. Mean WTP is significantly higher for private interventions for public[2] and community interventions. Altruistic mean WTP is much lower than overall mean WTP, again in line with our hypothesis. Treatment and prevention valuations are different with prevention showing higher valuation results.

5. Analysis

5.1. Replication of Trapero-Bertran et al. (2012)

We adjusted some data in order to replicate the results presented in Trapero-Bertran et al. (2012). We make monetary conversions by exchange rate, choosing 2012 as the target year (as opposed to 2011 in the previous study). This should not have any discernible impact on the relations between the covariates and the dependent variable. We still keep the per policy conversion, which was not done in the previous study. Also, we omit the double counts from our analysis, which is also different from the previous study.

Table 3 gives the replication output, where the reduced and extended models are in line with the specification in the previous study. The regression model is the weighted random effects model, as shown in Equation (2). This is the same model used in the previous meta-analysis. Thus, the only differences are the dataset (we have 39 more observations after omitting double-counts) and monetary conversion. We also include the regression results from Trapero-Bertran et al. (2012).

We largely fail to replicate the results reported in Trapero-Bertran et al. (2012). One result that we successfully reproduce include insecticide-treated nets being valued higher than other prevention goods or methods. Another is that the open-ended CV elicitation method yields lower WTP values than the bidding game method.

Table 2. What is the overall mean WTP?

	Weighted			Unweighted			
	Mean WTP	Lower CI	Upper CI	**Mean WTP**	Lower CI	Upper CI	**N**
Private (Goods)	37.987	36.396	39.578	52.010	38.201	65.818	144
Public (Services)	26.121	24.712	27.530	164.413	−5.899	334.725	44
Treatment	32.224	28.148	36.301	160.148	−45.915	366.211	36
Prevention	42.963	40.898	45.029	57.189	41.907	72.471	138
One-off payment	36.218	33.738	38.699	118.094	25.434	210.754	81
Annual payment	36.159	34.328	38.283	40.480	33.245	47.714	89
Altruistic	9.687	8.498	10.876	45.294	−22.149	112.738	23
Overall	**39.509**	37.680	41.339	**78.317**	37.671	118.963	188

Table 3. Replication and previous results.

	Our replication		Trapero-Bertran et al. (2012)	
	Replication base model	Replication reduced model	Base model	Reduced model
Insecticide treated net (baseline)				
Other prevention	−0.598*** (0.162)	−0.596*** (0.175)	−1.19*** (0.26)	−1.09*** (0.25)
Treatment	−1.019*** (0.208)	−0.916*** (0.221)	0.21 (0.30)	0.29 (0.27)
BG dummy (baseline)				
SBDC + OE dummy	−1.041*** (0.309)	−0.809*** (0.298)	−0.15 (0.32)	−0.14 (0.31)
SH dummy	−0.0364 (0.248)	−0.337 (0.262)	−0.04 (0.42)	−0.24 (0.38)
OE dummy	−0.438* (0.215)	−0.495* (0.230)	−1.76*** (0.38)	−1.70*** (0.33)
SBDC dummy	2.884*** (0.298)	2.678*** (0.310)	0.86 (0.52)	0.84* (0.5)
PC dummy	1.152** (0.446)	0.690 (0.473)	−0.94 (7.00)	−0.18 (0.66)
Not specified	−1.978*** (0.488)	−1.980*** (0.486)	0.19 (0.31)	0.14 (0.30)
Rural (baseline)				
Rural or urban	−1.193*** (0.263)		0.07(0.29)	
Zeros included in WTP dummy	−0.560*** (0.152)		−0.19 (0.30)	
Log GNI per Capita	0.280*** (0.0943)	0.116* (0.0665)	1.26** (0.50)	1.47*** (0.46)
Nigeria	−0.336 (0.218)		0.29 (0.30)	
Constant	1.616** (0.669)	1.337** (0.591)	0.18 (0.70)	0.20 (0.42)
N	140	140	101	101
τ^2	0.48	0.60	0.75	0.78
I^2 residual	99.41%	99.52%	99.35%	99.68%
Adjusted R^2	56.98%	47.18%	47.71%	44.79%

Notes: Standard errors in parentheses *($p<0.10$), **($p<0.05$), ***($p<0.01$). The regression results from Trapero-Bertran et al. (2012) are publicly available in the paper.

Although the databases are different (see Table A3 in Appendix), this underlines how sensitive results can be, especially in meta-analyses (Nelson and Kennedy 2009) and motivates the importance of conducting sensitivity analyses and subjecting the model to different regression specifications.

5.2. Main regressions

Table 4 gives the output of our regression analysis. Columns 1 and 2 are the WLS and weighted random effects models, respectively. Both regression models are weighted by the standard error of the mean WTP statistic. Column 3 presents the mixed effects model. It accounts for study and author effects, nullifying the need to weigh observations by standard errors. Column 4 presents the outcome of the mixed effects model without residual outliers. An outlier is defined as being more than two standard deviations away from the mean residual (Bellavance, Dionne, and Lebeau 2009). Both columns 3 and 4 have coefficients that have standard errors calculated by the Huber–White estimator to control for heteroskedasticity (White 1980).

A finding is consistent if we observe it throughout all the columns in Table 4. This means that the result is robust to different model specifications and not driven by outliers. We discuss the results from the mixed effects regression (column 3). The range of the coefficient, from the other models, is given in the parentheses.

In all columns, the dependent variable is the logarithm of mean WTP (log(mean WTP + 1)). The baseline category consists of studies on services, with one-time payment mechanism, that are not done in Nigeria and are stated preferences using the SBDC elicitation procedure.

- Income: The income elasticity is the coefficient of the income variable, since we use a log–log specification. The income elasticity is 0.52 (0.29–0.52), implying that a 1 per cent increase in household income, ceteris paribus, will lead to a 0.52 per cent increase in mean WTP to avoid malaria morbidity. This validates global climate change models that assume income elasticity for vector-borne diseases, such as malaria, to be less than 1 (Tol and Heinzow 2003; Bosello, Roson, and Tol 2006).

Table 4. Results across regression model specifications.

	(1) WLS	(2) Weighted Random Effects	(3) 2-Level Mixed Effects	(4) 2-Level Mixed Effects w/o Outliers
Altruistic dummy	−1.249*** (0.393)	−1.238*** (0.393)	−0.841*** (0.0498)	−1.402*** (0.0381)
Treatment dummy	−0.0930 (0.467)	−0.0932 (0.466)	−0.126 (0.107)	−0.134*** (0.0381)
Goods dummy	0.0583 (0.542)	0.0591 (0.540)	0.396*** (0.0158)	0.343*** (0.00864)
Technical Report Publication dummy	−0.263 (0.799)	−0.260 (0.799)	0.453 (0.535)	0.628 (0.512)
Mean Age	0.672* (0.344)	0.675* (0.343)	0.146*** (0.0475)	0.114*** (0.0320)
Log Mean Age	−32.30** (15.27)	−32.43** (15.23)	−4.572** (2.138)	−3.503** (1.483)
Log Standardized Income	0.293** (0.135)	0.294** (0.134)	0.518*** (0.0367)	0.343*** (0.0171)
Nigeria	1.153 (0.805)	1.159 (0.802)	−0.555 (0.699)	−0.306 (0.828)
Revealed Preferences dummy	−3.297*** (0.465)	−3.297*** (0.465)	−1.698*** (0.523)	−1.531*** (0.515)
Zeros Included in WTP dummy	−0.00385 (0.323)	−0.00443 (0.321)	0.187* (0.105)	0.266* (0.161)
One Month Payment dummy	−0.148 (0.735)	−0.143 (0.732)	−1.238* (0.719)	−1.046 (0.832)
Three Months Payment dummy	1.128 (1.410)	1.129 (1.406)	−1.984** (0.845)	−1.836* (1.054)
Yearly Payment dummy	−0.792*** (0.276)	−0.793*** (0.275)	−0.876*** (0.130)	−0.663*** (0.0464)
OE dummy	−2.182*** (0.444)	−2.187*** (0.442)	0.230 (0.330)	0.424*** (0.125)
BG dummy	−2.869*** (0.586)	−2.872*** (0.584)	0.0823 (0.347)	0.0193 (0.137)
SBDC + OE dummy	−3.497** (1.354)	−3.505** (1.349)	1.505* (0.903)	1.886* (1.074)
DBDC dummy	−2.647*** (0.577)	−2.651*** (0.576)	0.113 (0.343)	−0.0601 (0.136)
PC dummy	−4.509*** (1.250)	−4.517*** (1.246)	0.479 (0.902)	0.832 (1.074)
Not Specified	−9.793*** (2.256)	−9.750*** (2.252)	−1.964*** (0.625)	−2.372*** (0.567)
Constant	95.74** (42.62)	96.09** (42.51)	10.44* (5.886)	9.052** (4.259)
Author RE Constant			1.035 (0.222)	1.145 (0.203)
Study RE Constant			3.62e−10 (3.41e−08)	6.93e−12 (9.18e−10)
Overall Residuals Constant			0.325*** (0.0425)	0.201*** (0.0159)
Observations	64	64	97	91

Notes: Standard errors in parentheses. *(p<0.10), **(p<0.05), ***(p<0.01).

- Age: As expected, the impact of age is non-linear. The regressions show that people above the age of 31.32 (30.73–48.07) have increasing WTP with each passing year. Before this cut-off, the marginal impact of age is negative. The age variable ranges from 24 to 58, with an average of 40. Hence, most of our sample is above, or close to, the cut-off. This is in support of the VSL literature findings (Krupnick 2007). To see an illustration of the impact of age, and a justification of the linear and logarithmic specification, see Figure A1 in the appendix to this paper.
- Altruism: The coefficient on the altruism dummy is −0.84 (−0.84 to −1.40). This indicates that altruistic policies, on average are valued 56.9% (56.9–75.3%) less than non-altruistic policies. This suggests that agents may free-ride on public goods, reflected in the decrease of mean WTP. Intuitively, non-altruistic policies should mainly concern goods, do to their rival nature. However we find no correlation between altruistic policies and goods through a chi-squared test (p-value > .1)[3], alleviating the need to add an interaction term.
- Revealed Preferences: The coefficient of the revealed preferences dummy is −1.70 (−1.53 to −3.30). This indicates that revealed preference studies, as opposed to stated preference studies produce 81.8% (78.3–96.4%) lower WTP values. This result should be approached with caution, since only one study (Hoffmann, Barrett, and Just 2009) provides revealed preferences data.
- Payment Frequencies: Mean WTP for annual payments is, as expected, lower than one-off payments. We find no consistent impact of monthly and quarterly payments. Annual payments lower mean WTP by 58.4% (48.5–58.4%). This is consistent with the environmental valuation literature findings, indicating that respondents discount future benefits (Loomis and White 1996; Pearce et al. 2002).

We find no consistent differences between treatment and prevention. Although we have significant treatment coefficients in some regression models, these are not consistent throughout the entire analysis. Indeed, the difference is negative in some specifications and positive in others. We find similar inconsistent results for the inclusion of zeros, if the study was carried out in Nigeria, goods instead of services and if the publication was a technical report instead of a journal article.

Heterogeneity due to author and study random effects are controlled for in the mixed effects regressions. When author effects are taken into account, the reported study effects become very small in magnitude. This provides evidence that any heterogeneity across studies by the same (corresponding) authors is driven by the researchers.

Although the mixed effects regression is put forward as the most preferable specification due to its flexibility, this is not enough to declare it as a strictly superior model. In order to compare the regression models, we apply another metric, namely its predictive power. Therefore, if a model is 'better' than another one, this means that is more suitable for using in predictions. A numerical comparison is done in the next subsection.

5.3. Comparison of model predictive power

We perform an out-of-sample test as a cross-validation exercise (Osborne 2000). Many out-of-sample tests have been developed and are widely used in meta-analysis literature (e.g. Brander and Florax and Vermaat 2006; Lindhjem and Navrud 2008; Vista and Rosenberger 2013). To the best of our knowledge, this particular test is the first implementation in the literature. The cross-validation exercise is done as follows. A random 80 per cent of the sample is taken and the regression models from equations 1 to 3 are run, just like the first three columns in Table 4. The result of these regressions are then used to predict the mean WTP values of the remaining 20 per cent of the sample. Then, the root mean sqaured error is calculated, as such:

$$\sigma_p = \sqrt{\frac{\sum_{i \in N} (Y_i - \hat{Y}_i)^2}{N}},$$

where N is the size of the test sample. Y_i denotes the observation while $\hat{Y}_i$ denotes the predicted value. Therefore, a lower σ_p constitutes a higher predictive power. This is similar to the leave-one-out test done in many meta-analyses (e.g. Brander et al. (2012)).

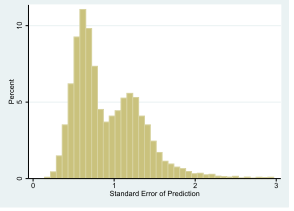
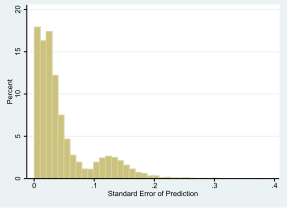
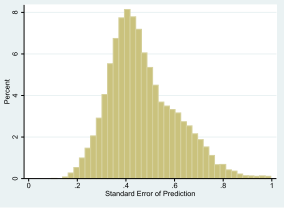
For each regression model, this exercise was repeated 10,000 times in order to get a distribution of the σ_p for each model. Before every run, the control (80 per cent) and test samples are determined randomly. A variable, which changes across observations with the uniform distribution between 0 and 1, is created for all observations. All observations below 0.1 or above 0.9 are considered the test sample, while the others are considered the control sample. Once the σ_p is calculated and stored, the variable with the uniform distribution is deleted, re-created and the process is repeated.

In Table 5, we can see the distribution of the root mean squared error(σ_p) per model, along with some distribution statistics.

At first glance, the weighted random effects outperforms the mixed effects in terms of predictive power. The average σ_p is smaller by an order of 10 (0.049 versus 0.472). However, the maximum σ_p calculated with the mixed effects is almost half the one for the weighted random effects (2.412 versus 1.444). Hence, mixed effects does better in minimizing the maximum error made. For conducting benefit transfers, the test statistic suggests that the weighted random effects model is more suitable than the other models.

Even though the mixed effects does better in terms of model flexibility, as shown in Section 2, and in minimizing the maximum prediction error. The weighted random effects model does better in minimizing the overall prediction error.

Table 5. Cross-validation results across model specifications.

Model	WLS	Weighted random effects	Mixed effects
			
Average σ_p	0.940	0.049	0.472
Std Deviation	0.473	0.078	0.144
Minimum σ_p	0.142	0.0004	0.093
Maximum σ_p	12.952	2.412	1.444

One reason why the mixed effects could be preferred as a benefit transfer function specification is the distribution of the σ_p values in Table 5. They are more symmetric-looking than the weighted random effects, making any benefit transfer error easier to handle.

6. Conclusion and discussion

Our main research focus is to see what explains mean WTP to avoid morbidity risk due to malaria. The research question implies looking only at malaria prevention, but we look at malaria treatment as well. We improve the analysis, with respect to the initial study (Trapero-Bertran et al. 2012), by using better data, having stricter variable definition standards, implementing a more comprehensive regression analysis and reaching new conclusions.

Our overall contribution can be split into four parts. We make use of more variables in the database and make sure to eliminate double-counts. By using a more detailed database, we control for important variables of interest and reduce potential omitted variable bias. This includes PPP conversions, payment frequency dummies and identifying potential pre or post morbidity differences in valuation. We consider the sensitivity of results in meta-regressions as an issue (Nelson and Kennedy 2009) and hence use different regression specifications, robustness checks and cross-validation. Lastly, we find consistent results, of which some are new to the morbidity literature, in particular the impact of age on mean WTP and lower mean WTP for altruistic policies rather than privately consumed goods.

6.1. Conclusion

We hypothesize that income will have a positive effect, while revealed preferences and having more frequent payments will have negative effects. These three hypotheses are supported by the regression analysis. Additionally, Nigeria and the inclusion of zero WTP values are shown to have no impact, confirming the findings in Trapero-Bertran et al. (2012).

Differences across CV elicitation methods are not observed consistently. This is unexpected, since different CV elicitation methods are known to give different results (Bateman et al. 1995). If we observe a significant effect with less residual noise, this indicates that the true effect might not be detected in the other models due to high residual variance. This explanation is supported by our small sample size. The literature suggests there are differences, but our sample size may not be big enough to detect them.

We also add new hypotheses based on mainstream economics and the mortality literature. One of these hypotheses is that age has a non-linear impact on mean WTP. This hypothesis finds support in the fact that the regressions show a consistent non-linear impact of age on mean WTP. The positive portion of this impact, valid for people above 30 years of age, is under discussion in the literature.

One explanation is that this age is where most families look after children who are more vulnerable to the effects of malaria than adults (Krupnick 2007). Since the data on the number of children per household is incomplete, we were unable to include more control variables in the regression models, thus we cannot check for this.

Another new hypothesis is that altruistic policies are valued less than non-altruistic ones because of free-riding. The regression analysis supports this hypothesis, suggesting that altruistic policies are treated more as public goods by the households. This result could also be due to protest voters, since the household may be seeing community policies as the responsibility of the government (Fonta, Ichoku, and Kabubo-Mariara 2010). However, the protest zero rates are not explicitly reported in all papers, thus we cannot check or control this possibility.

Health-state dependent expected utility theory implies that WTP should not change with respect to an agent's initial health status (Viscusi and Evans 1990). In other words, pre and post morbidity valuation should not be different from each other, because the sick state utility function does not consider reference points to be important. Alternatively, prevention can be valued more than treatment due to projection bias (Loewenstein, O'Donoghue, and Rabin 2003; Dolan and Kahneman 2008). Since we fail to find that treatment and prevention WTP's are statistically different, our results are supportive of the mainstream expected utility theory. However, since the households in these studies are from malaria endemic areas, it is not unlikely that they have some idea of the discomforts of malaria, hence have less projection bias. We cannot see what happens in non-malaria endemic areas, since there are no studies conducted outside endemic areas.

Based on the mortality literature, WTP to avoid mortality risks can change with how much perceived control the respondent has over the particular risk (Viscusi, Kip, and Huber 1987; Viscusi, Magat, and Huber 1991). We test this result in malaria morbidity valuation by looking for differences in WTP between goods and services. The regression analysis fails to show consistent statistical significant difference. It should be noted though that the mixed effects models show that goods are valued, on average, more than services. However, this is not evident in the other regression specifications.

6.2. Discussion

It is clear that this paper is not a reply to Trapero-Bertran et al. (2012). Though this previous study provides a reference point, the approaches are fundamentally different. We compare results across three different regression models, do a sensitivity check and a novel test of predictive power. Trapero-Bertran et al. (2012) considers one regression model and varies it by the number of covariates. Their analysis is not carried further. Moreover, we are not able to replicate their results, due to slight differences in the datasets used. This is telling of how sensitive results are to the dataset used in any empirical work, especially meta-analyses, thus requiring rigorous robustness checks.

An extension of our robustness checks becomes a cross-validation to select the best-performing model. As shown in Table 5, the weighted random effects was superior in prediction but mixed effects had a better dispersion of prediction error. This calls for a careful selection of distributional assumptions made on the error term in transfer functions. A symmetric distribution can be better justified for the mixed effects than for the weighted random effects function. Further studies can also integrate cross-validation to fine-tuning variable selection in transfer functions.

Our conclusions and contributions are constrained in a number of ways. First of all, the results here are valid for malaria endemic areas only. We can try to transfer these benefit values to non-malaria endemic areas, but the prediction error is expected to be high. Another limitation is that we do not have enough data from the papers to further explain our main results. We only identify significant effects to mean WTP but are limited in explaining the channels through which these effects occur. A clear example is age, where we have a consistent result but no clear channel. Hence, the analysis here is open to extensions and improvements.

Notes

1. Legesse et al. (2007) and Adepoju, Ogunmodede, and Oyekale (2012).
2. The stated preference studies collected focused on private and public policies in preventing/treating malaria. Public policies always included services (e.g. spreading pesticide) and private policies always included goods (e.g. bednets). Since the incentive structures are different for publis and private policies, we separate them through a dummy in the regression models. Public (private) policies are referred to as services (goods) due to the previous explanation.
3. The test was run for the sample used in the regression models.

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Appendix

In the regressions, age enters as a linear and logarithmic term of the equation. In their review, Krupnick (2007) concludes that age has a quadratic relationship with health valuation. The same study also concludes that this captures the declining positive impact of age as one gets older. We show the analytical differences of the impact of age with equations below. Hereon, Y denotes the logarithm of mean WTP and X denotes age. Consider a quadratic specification, as concluded in Krupnick (2007):

$$Y = \alpha_1 X^2 + \alpha_2 X.$$

The impact of X on Y is given by the first derivative, $2\alpha_1 X + \alpha_2$. Prior evidence suggests that this term should be positive, but diminishing as age increases. Therefore α_2 is positive and α_1 is negative. The impact of increasing age is not expected to be negative, therefore the α_1 term has a small magnitude. This means that, with a small sample, α_1 may not be detectable.

This motivates the introduction of the logarithmic term. The above equation now becomes, as it is in our regressions

Table A1. Contingent valuation main elicitation methods.

Abbreviation	Expansion	Definition
OE	Open Ended Question	Respondents are asked what their maximum WTP is for the given good or service in an open ended questions (Onwujekwe and Uzochukwu 2004).
PC	Payment Card	Respondents are given a card with various values on them, to help them find and state their maximum WTP by circling the relevant value (Masiye and Rehnberg 2005).
SBDC	Single Bounded Dichotomous Choice	Respondents are given a price value and are asked if they are willing to pay this amount of money or not. They only answer yes or no. After that, binary regression techniques (logit, probit) are used to estimate a bid function from which WTP is derived (Hanemann, Loomis, and Kanninen 1991).
SBDC + OE	Single Bounded Dichotomous Choice + Open Ended Question	SBDC with an open ended maximum WTP follow-up question (Fonta, Ichoku, and Kabubo-Mariara 2010).
DBDC	Double Bounded Dichotomous Choice	Depending on the answer to the SBDC (yes or no), the respondent is presented in the DBDC elicitation format either a higher (if response to first bid was yes) or lower (if response to first bid was no) value and asked if they are willing to pay this amount of money. The sequence of WTP questions in a DBDC elicitation format is also sometimes referred to as a bidding game which can continue also to a third level.
BG	Bidding Game	Respondents are faced with a discrete price question. If they answer yes, then the price increases and the same question with the new price is asked. If they had answered no, then the price decreases and the same question with the new price is asked. Once the respondent answers yes/no after no/yes, the price is assumed to be their maximum WTP (Mitchell 1989).

$$Y = \alpha_1 log(X) + \alpha_2 X.$$

The impact of age is again given by the first derivative, $\alpha_2 + \alpha_1(1/X)$. The term, $\alpha_1(1/X)$, decreases in magnitude as age increases. This means that the α_2 value, in absolute terms, is larger. This makes it more likely to be detected.

Indeed, this is what we find when the regressions are run with the quadratic and the linear + logarithmic specifications. In the quadratic specification, both α_1 and α_2 are not significant, but have the expected signs. In the linear +

Table A2. Descriptive statistics of mean WTP.

	Weighted			Unweighted			
	Mean WTP	Lower CI	Upper CI	Mean WTP	Lower CI	Upper CI	N
Private (Goods)	37.987	36.396	39.578	52.010	38.201	65.818	144
Public (Services)	26.121	24.712	27.530	164.413	−5.899	334.725	44
Treatment	32.224	28.148	36.301	160.148	−45.915	366.211	36
Altruistic	9.687	8.498	10.876	45.294	−22.149	112.738	23
Nigeria	47.586	44.067	51.104	51.756	35.742	67.770	97
Revealed preferences	16.261	14.219	18.304	31.974	12.901	51.047	7
Zeros included	37.997	35.808	40.186	66.175	43.853	88.497	138
One off payment	36.218	33.738	38.699	118.094	25.434	210.754	81
Monthly payment	132.554	97.063	168.045	138.332	−2.471	279.135	11
3-Monthly payment	4.383	2.498	6.268	4.803	2.638	6.969	7
Yearly payment	36.159	34.328	38.283	40.480	33.245	47.714	89
Journal publication	39.410	37.503	41.317	79.078	35.599	122.557	175
Technical report publication	34.093	18.981	49.205	95.371	−78.484	269.226	6
SBDC	358.921	231.940	485.902	177.763	94.629	260.896	19
SBDC + OE	9.955	3.601	16.309	9.955	5.278	14.631	8
OE	13.654	9.725	17.584	19.122	11.274	26.971	23
BG	34.044	30.805	37.283	41.135	30.516	51.755	89
DBDC	56.695	52.863	60.527	72.537	26.785	118.289	33
PC	3.016	2.334	3.699	179.544	−225.229	584.317	6
Not specified	1.506	1.004	2.007	10.011	−8.958	28.980	6
				Average	Lower CI	Upper CI	N
Household income				7710.058	4523.344	10896.77	114
Respondents' age				40.809	39.701	41.910	161
Nigeria							97
Ethiopia							13
Overall mean WTP	39.509	37.680	41.339	78.317	37.671	118.963	188

logarithmic specification, all coefficients are significant, with the expected signs. A visual interpretation of the modelled relationship between age and mean WTP can be found in Figure A1.

Table A3. Studies used in meta-analysis.

Reference	Country	Elicitation	Policy Type	Good/ Service	In Trapero-Bertran et al. (2012)
Adeneye et al. (2014)	Nigeria	SBDC	Prevention	Good	No
Alaii et al. (2003)*	Kenya	OE	Prevention	Good	Yes
Aleme, Girma, and Fentahun (2014)**	Ethiopia	OE	Prevention	Good	No
Asafu-Adjaye and Dzator (2003)**	Ghana	BG	Insurance	Service	Yes
Asante and Asenso-Okyere (2003)	Ghana	BG	Prevention	Service	No
Badjan (2011)**	The Gambia	BG	Prevention	Good	No
Bhatia and Fox-Rushby (2002)*	India	BG	Prevention	Good	No
Bhatia (2005)*	India	BG	Prevention	Good	Yes
Binam, Onana, and Nkelzok (2004)**	Cameroon	BG	Treatment	Service	No
Chase et al. (2009)	Mozambique	BWFU	Prevention	Good	Yes
Cropper et al. (2004)**	Ethiopia	SBDC	Prevention	Good	Yes
Dupas (2014)**	Kenya	OE	Prevention	Good	No
Fonta, Ichoku, and Ogujiuba (2010)**	Cameroon	SPC	Prevention	Service	No
Fonta, Ichoku, and Kabubo-Mariara (2010)**	Cameroon	SBDC+OE	Prevention	Service	Yes
Gebresilassie and Mariam (2011)**	Ethiopia	BWFU	Prevention	Good	No
Gunasekaran et al. (2009)	India	OE	Prevention	Good	Yes
	Kenya	SBDC+OE	Prevention	Good	Yes

(*Continued*)

Table A3. Continued.

Reference	Country	Elicitation	Policy Type	Good/ Service	In Trapero-Bertran et al. (2012)
Guyatt, Ochola, and Snow (2002)*					
Hansen et al. (2013)*	Uganda	BG+OE	Detection	Good	No
Hanson et al. (2005)	Zambia	Choice Experiment	Treatment	Service	No
Hoffmann, Barrett, and Just (2009)**	Uganda	Auction	Prevention	Good	Yes
Jima et al. (2005)**	Ethiopia	OE	Prevention	Good	Yes
Jimoh et al. (2007)*	Nigeria	BWFU	Treatment, Prevention, Eradication	Good, Service	Yes
Lin et al. (2000)	Myanmar	OE	Prevention	Good	Yes
Masiye and Rehnberg (2005)**	Zambia	PC	Treatment	Service	Yes
Mboera et al. (2014)	Tanzania	SBDC	Prevention	Service	No
Mills et al. (1994)*	The Gambia	OE	Prevention	Good	Yes
Morey, Sharma, and Mills (2003)**	Nepal	Not Specified	Treatment	Service	No
Mujinja, Makwaya, and Sauerhborn (2004)*	Tanzania	SBDC+OE	Prevention	Good	Yes
Lertmaharit, Kamol-Ratanakul, and Saul (2000)	Myanmar	BG	Detection	Good	No
Okrah et al. (2002)	Burkina Faso	Not Specified	Prevention	Good	Yes
Onwujekwe et al. (2000)*	Nigeria	BG, BWFU	Prevention	Good	No
Onwujekwe (2001)*	Nigeria	BG, BWFU	Prevention	Good	Yes
Onwujekwe et al. (2001)*	Nigeria	BG, BWFU	Prevention	Good	Yes
Onwujekwe and Nwagbo (2002)*	Nigeria	BG	Prevention	Good	Yes
Onwujekwe et al. (2002)*	Nigeria	OE	Prevention	Good	No
Onwujekwe, Hanson, and Fox-Rushby (2003)**	Nigeria	BG, BWFU, SH	Prevention	Good	No
Onwujekwe, Hanson, and Fox-Rushby (2004)	Nigeria	Not Specified	Prevention	Good	Yes
Onwujekwe (2004)**	Nigeria	BG, BWFU, SH	Prevention	Good	Yes
Onwujekwe and Uzochukwu (2004)**	Nigeria	OE, BWFU	Prevention	Good	Yes
Onwujekwe et al. (2004)**	Nigeria	BG, SH	Treatment	Good	Yes
Onwujekwe, Fox-Rushby, and Hanson (2004)**	Nigeria	BG, BWFU, SH	Prevention	Good	No
Onwujekwe, Fox-Rushby, and Hanson (2005)*	Nigeria	BG, BWFU, SH	Prevention	Good	Yes
Onwujekwe et al. (2005)**	Sudan	BG	Prevention	Service	Yes
Onwujekwe et al. (2006)**	Nigeria	BG	Treatment	Service	Yes
Onwujekwe et al. (2007)**	Nigeria	BG	Treatment	Service	Yes
Onwujekwe, Fox-Rushby, and Hanson (2008)**	Nigeria	BG, BWFU, SH	Prevention	Good	No
Poulos (2000)**	Tanzania	SBDC	Prevention	Good	Yes
Prabhu (2010)**	India	SBDC	Prevention	Good	Yes
Rennie et al. (2009)*	Benin, Tanzania, Peru	BG	Detection	Good	Yes
Sauerborn et al. (2005)	Burkina Faso	BG	Prevention	Good	Yes
Udezi, Usifoh, and Ihimekpen (2010)**	Nigeria	PC	Prevention	Good	Yes
Uzochukwu et al. (2010)*	Nigeria	BG	Detection	Good	Yes
Weaver et al. (1993)	Central African Republic	SBDC	Treatment	Good	Yes
Whittington, Pinheiro, and Cropper (2003)	Mozambique	SBDC	Prevention	Good	Yes
Wiseman et al. (2005)*	Tanzania	BG	Treatment	Good	Yes

*denotes whether or not the study is in the Trapero-Bertran et al. (2012) meta-regression replication attempt.**denotes whether or not the study is in the replication attempt and 2-level mixed effects regression.

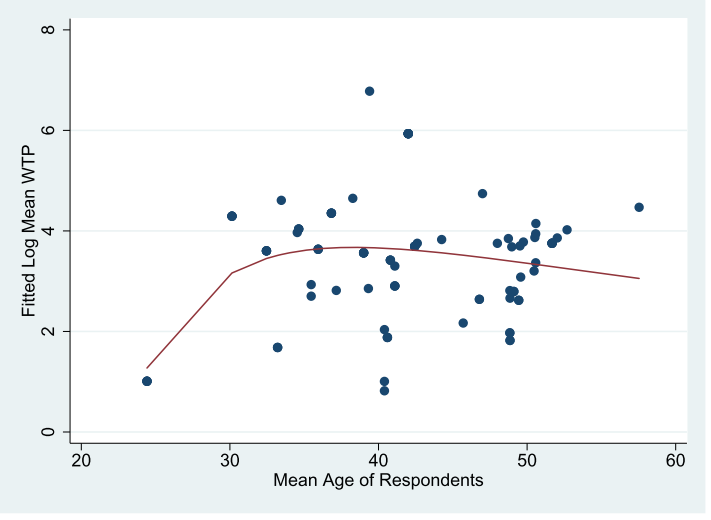

Figure A.1. Age vs Log Mean WTP.